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THE WAGE IMPACT OF THE MARIELITOS:
ADDITIONAL EVIDENCE

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The Wage Impact of the Marielitos: Additional Evidence
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ABSTRACT

Cards (1990) study of the Mariel supply shock is an important contribution to the literature that measures the labor market impact of immigration. My recent reappraisal (Borjas, 2015) revealed that even the most cursory reexamination implied that the wage of low-skill (non-Hispanic) working men in Miami declined substantially in the years after Mariel. In the three months since the public release of my paper, there has already been one “re-reappraisal” of the evidence. Peri and Yasenov (2015) make a number of alternative methodological choices that lead them to conclude that Mariel did not have a wage impact. This paper isolates the source of the conflicting results. The main reasons for the divergence are that Peri and Yasenov calculate wage trends in a pooled sample of men and women, but ignore the contaminating effect of increasing female labor force participation. They also include non-Cuban Hispanics in the analysis, but ignore that at least a third of those Hispanics are foreign-born and arrived in the 1980s, further contaminating the calculated wage trend. And, most conspicuously, they include “workers” aged 16-18 in the sample. Because almost all of those “workers” are still enrolled in high school and lack a high school diploma, this very large population of high school students is systematically misclassified as high school dropouts. This fundamental error in data construction contaminates the analysis and helps hide the true effect of the Mariel supply shock.

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The Wage Impact of the *Marielitos*: Additional Results

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1. Introduction

Card's (1990) study of the Mariel supply shock remains an important contribution to the literature that attempts to measure the labor market impact of immigration. Despite its significance, there have been remarkably few attempts in the past 25 years to carefully examine some of the assumptions and data manipulations that led Card to conclude that the *Marielitos* had little effect on the wage of Miami's workers.

Borjas (2015; henceforth GB15) represents the first systematic effort to reappraise the Mariel evidence. The key insight of that paper was that even the most cursory reexamination of the Mariel data contradicted Card's evidence: the wage of low-skill workers in Miami seemed to decline substantially in the years after Mariel.

In the short time since the public release of GB15 in September 2015 (although drafts of the paper were circulated privately during the preceding summer), its contradictory evidence has already inspired a "re-reappraisal" of the evidence. Peri and Yasenov (2015; henceforth PY) introduce a number of alternative methodological choices that successfully "resurrect" Card's conclusions. PY further claim that their approach is preferable, so that from their perspective the case is closed and Mariel did not have a wage impact.

This addendum to my Mariel reappraisal isolates the specific assumptions and data manipulations that lead to the contradictory results in GB15 and PY. The two papers differ in numerous ways, but there are four key differences that account for the conflicting results.

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Each difference is typically not sufficient to turn the strong negative impact documented in my paper into the zero wage effect that PY report, but their cumulative impact explains practically all of the variation. The differences are:

1. The choice of a different set of placebo cities. Expanding on my application of the Abadie, Diamond, and Hainmueller (2010) synthetic control method, PY use a different set of matching variables to construct an alternative control. The different matching variables generate a placebo that shrinks the post-Mariel wage gap between Miami and the control cities. This difference between the GB15 and PY studies, however, plays the least important role in creating the divergent results.
2. PY argue that the wage trends for low-skill workers in the various cities should be calculated in a sample of high school dropouts that includes both men and women. The wage trends of men and women differed radically both in Miami and in the control cities during the 1980s. The female wage trend, however, is contaminated by the rapidly increasing labor force participation rate of women in the low-skill workforce. This rare pooling of men and women “dilutes” some of the steep wage decline observed among low-skill men.
3. GB15 limited the wage analysis to non-Hispanics so as to better approximate the trends in the native-born workforce. PY argue that the wage trends should be calculated in a sample of workers that includes the large Hispanic population. The 1980s witnessed the entry of a large number of Hispanic immigrants, and the inclusion of these new entrants contaminates wage trends substantially, particularly in some of the comparison cities.
4. GB15 examined a sample of workers aged 25-59. PY argue that the sample should include workers aged 16-61. This change introduces a fundamental error in PY’s analysis. Typically, we think of high school dropouts as persons who *failed to finish* high school. PY define a “high school dropout” as someone who has not completed

high school. The inclusion of 16-18 year-olds in the data, the vast majority of whom are still enrolled in high school, implies that *high school sophomores, juniors, and seniors are systematically misclassified as high school dropouts*. This misclassification plays the most important role in hiding the wage differences that do exist between working adults in post-Mariel Miami and the comparison cities.

This paper addresses each of these four issues separately. By taking this approach, I hope to show—in as transparent a way as possible—how the very strong negative wage effect documented in GB15 turns into the zero wage effect claimed in PY. As in GB15, the analysis uses data from both the March and the Outgoing Rotation Group (ORG) files of the Current Population Surveys (CPS).

2. The Choice of a Placebo

As I pointed out in GB15, Card's (1990) construction of a placebo made an elementary error in experimental design. Card's description is less than precise, but his choice of the placebo cities (Atlanta, Houston, Los Angeles, and Tampa) was based on a comparison of employment trends in Miami and other cities between the mid-1970s and the mid-1980s. It is obviously incorrect to select a placebo based on similar *post-treatment* employment patterns in the treated group and the control group.

GB15 introduced two alternative placebos. The first was composed of cities that had almost identical employment growth as Miami in the pre-Mariel period. The cities in this "employment placebo" were Anaheim, Rochester, Nassau-Suffolk, and San Jose. In addition, GB15 applied the synthetic control method to the Mariel context, matching cities on the

basis of similar wage and employment growth prior to 1980. The cities with sizable weights in this synthetic control were San Diego, Anaheim, Rochester, and San Jose.

PY also employ the synthetic control method, but choose different variables to do the matching. Their variables are: the city's log weekly wage, the share of high school dropouts, the share of Hispanics, and the share of workers in manufacturing. The cities with non-zero weights in the March CPS placebo were Dallas, Los Angeles, and New York (PY, Table 3). The respective cities in the ORG analysis were Philadelphia, Tampa, Birmingham, and Anaheim.

Note that the cities chosen by the synthetic control method differ depending on the sample, the variables used to conduct the matching, and so on. To simplify the discussion, and to make the empirical analysis transparent, I use three alternative placebos throughout this paper regardless of sample composition: The "Card placebo," the "employment placebo" from GB15, and the four cities (Philadelphia, Tampa, Birmingham, and Anaheim) that PY's application of the synthetic control method picked in the ORG data.

I start the empirical analysis by first reproducing the results from GB15. The specific sample used was: non-Hispanic men, aged 25-59, not enrolled in school, and employed in the wage and salary sector. The dependent variable is the log weekly wage, and it is adjusted for age differences among workers (see GB15, p. 18). The top panel of Figure 1 duplicates the results from GB15, except that it also reports the trend in the set of cities that make up the PY placebo. Note that the use of the PY placebo does not alter any of the key visual insights from my earlier analysis. The PY placebo tracks the Card placebo in the March CPS data, and tracks the employment placebo in the ORG. In the end, the choice of

the placebo does not really matter all that much when we focus on wage trends among prime-age men.

Panels A and B of Table 1 reprint the basic set of regressions from GB15, except that the last column reports the regression coefficients when using the PY placebo. The regression model calculates difference-in-differences estimates of the wage impact for three-year periods relative to the pre-1980 wage. The use of the PY placebo tends to shrink the estimated wage impact of the Marielitos, but there is still a significant wage effect. In the ORG, the immediate (first three years) wage effect is -0.153 (with a standard error of 0.060) when I use the employment placebo and -0.131 (0.033) when I use the PY placebo. The fact that placebo choice does not matter all that much is not surprising. As I showed in GB15 (Table 8), Miami's relative wage decline was negative and statistically significant when compared to what happened in 95 percent of *all* 123,410 potential four-city placebos.

A discussion of which matching variables should be used to select the synthetic control is beyond the scope of this paper, but a warning is warranted. It is evident that different choices may lead to very different results, and it is often difficult to ascertain whether a particular variable should be used in the matching algorithm. For example, one could easily argue that matching Miami with cities that had large Hispanic populations in the 1970s may not be sensible. In what sense are the labor market conditions in predominantly Cuban Miami similar to the conditions in predominantly Mexican Los Angeles or predominantly Puerto Rican New York *circa* 1980?

Despite these concerns, it turns out that the choice of a placebo, although it might tend to somewhat shrink or magnify wage effects, is the least important of the four

differences outlined earlier. The changes in sample composition play a much more important role in generating the discrepancy between my findings and PY.

3. The Role of Gender

The bottom two panels of Figure 1 illustrate what happens when the sample is expanded to include women. It is obvious that the wage trends for women differ markedly from those for men, both in Miami and in the placebo cities. Given these differences, it is inevitable that the pooling of men and women leads to different results than reported in GB15. Most strikingly, the U-shape wage trend in Miami revealed by the March CPS during the 1980s tends to disappear. In the ORG data, the apparent decline in the relative wage of Miami's low-skill workers is also greatly attenuated.

Panels C and D of Table 1 report the regression coefficients when the sample includes working women.¹ Interestingly, the regression coefficients change dramatically in the ORG sample, but change far less in the March CPS (more on this below). If I use the PY placebo, the immediate wage impact in the ORG falls from -0.131 (0.033) in the sample of men to -0.006 (0.054) in the pooled sample of men and women. In the March CPS, however, the relative wage impact using the PY placebo is -0.150 regardless of whether the data includes women or not.

The relevant question obviously becomes: Should women be included in the sample? In GB15, I argued that: "It is tempting to increase sample size by including working women in the study, but female labor force participation was increasing very rapidly in the

¹ The regression pools male and female workers, adjusts for age, and adds a gender dummy variable. The age-adjusted wage is evaluated at the mean percent male in the pooled data.

1980s, so that wage trends are likely to be affected by the selection that marks women's entry into the labor market." In fact, the labor force participation of non-Hispanic, low-skill women aged 25-59 increased differentially between 1980 and 1990 in the various cities: from 56 to 62 percent in Miami, 51 to 53 percent in the Card placebo, 51 to 56 percent in the employment placebo, and 47 to 54 percent in the PY placebo.

The problem with including women in the calculation of the average wage does not only arise because of the increase in female participation rates, but also because of the intermittent nature of such participation. Even if the participation rate had remained constant over time, the sample of women who are actually working would likely change in unknown ways across different cities that offer different opportunities.²

The differential impact of the inclusion of women in the March CPS and the ORG data files illustrates the significance of these concerns. The women included in the March CPS data are women who worked *at any time during a calendar year*, while the women included in the ORG are women who worked *during a randomly chosen week*. Given the flow of women in and out of the labor market, the March CPS may generate a more representative sample of women than the ORG, and this might explain why the wage effects in the March CPS are much more robust to the inclusion of women.

I suspect most economists would be wary of examining and making inferences from wage trends when the sample is changing dramatically over time both because of a composition effect (i.e., there are many more working women in the data in later than in

² It is tempting to argue that the difference-in-differences strategy gets rid of the selection bias. However, there is no reason to presume that the selection bias term in Heckman-style earnings functions vanishes if we differenced the wage trend in Miami from that in the placebo. Note also that low-skill men and women also had very different occupational profiles in the 1980s, and almost two-thirds of low-skill *Marielitos* were men. It is doubtful that the *Marielitos* were competing in the same labor market as low-skill women.

earlier years), and a selection effect (i.e., the unobserved characteristics of working women are changing over time). Despite the fact that the study of this type of selection bias has been a central concern in labor economics since the 1970s, PY do not make any attempt at getting rid of the data contamination. In fact, any such attempt would raise even more questions because there are few robust ways of purging the data from the selection bias.

The excuse that including women in the sample increases sample size is *not* a good excuse if it leads to contaminated data. There is a good reason why some of the classic studies in the wage structure literature focus exclusively on male wage trends (Murphy and Welch, 1992; Card and Lemieux, 2001). And even those studies that examine female trends do *not* pool wage data for men and women, and instead calculate the wage trends separately (Katz and Murphy, 1992; Lemieux, 2006; Autor, Katz, and Kearney, 2008). It is unwise to ignore the lessons of three decades worth of research on female labor supply and earnings in the search for a larger sample size in the Mariel context.

4. The Role of Hispanic Immigrants

To further increase sample size, PY argue that the sample of workers should include not only women, but also all non-Cuban Hispanics. I excluded Hispanics in GB15 for a simple reason: The 1980s were a period of rapid growth in Hispanic immigration, and the wage trend calculated in a sample where the composition is systematically changing due to the entry of many low-skill immigrants is not going to tell us much about the impact of Mariel.

Before providing some statistics on low-skill Hispanic immigration during the period, let me first summarize what happens to the observed wage trends in Miami and the

various placebos when the data are expanded to include (non-Cuban) Hispanics. Figure 2 and Table 2 present the relevant wage trends and regression coefficients.

It is obvious that the inclusion of Hispanics greatly distorts the trends in the various cities. Interestingly, the trend in the Miami labor market is relatively unchanged because the vast majority of Hispanics in Miami at the time were Cuban immigrants (and both GB15 and PY agree that Cubans should not be in the comparison group). The inclusion of Hispanics, however, does affect the wage trends in all the placebos. The regressions suggest that the inclusion of Hispanics attenuates the wage impact of the *Marielitos*. For instance, the immediate wage impact in the ORG when using the PY placebo falls from $-.131$ (.034) when the sample excludes Hispanics to -0.077 (0.038) when the sample includes Hispanics.

The question again becomes: should Hispanics be included in the sample to calculate wage trends? Ideally, the empirical analysis would examine wage trends among native-born workers. This does not present a problem when using decennial census data, where birthplace information is readily available. As a result, few studies that use census data to analyze supply shocks examine the pooled earnings of comparable natives and immigrants.³

Unfortunately, the CPS did not systematically collect information on country of birth until 1994, making it impossible to carry out the ideal analysis. I specifically noted in GB15 that the restriction of the sample to non-Hispanic men was one way of ensuring that the wage trends in Miami best approximated the wage trend of native workers:

³ Ironically, Ottaviano and Peri (2012) argue that those two groups of low-skill workers are *not* perfect substitutes and should *not* be pooled.

The CPS did not report a person's country of birth before 1994...I instead examine the impact on non-Hispanic men..., a sample restriction that comes close to identifying Miami's native-born population at the time. For example, the 1980 census, conducted days before the Mariel supply shock, reports that 40.7 percent of Miami's male workforce was foreign-born, with 65.1 percent of the immigrants born in Cuba and another 11.2 percent born in other Latin American countries.

The inclusion of Hispanics affects the wage trends so markedly for a very simple reason: there was a sizable increase in low-skill Hispanic immigration during the 1980s. As a result, 78 percent of the (non-Cuban) Hispanics in Miami in 1990 were foreign-born, and 63 percent of them had arrived in the 1980s. In the Card placebo, 85 percent were foreign-born and 43 percent had arrived in the 1980s. In the employment placebo, 78 percent were foreign-born and 51 percent had arrived in the 1980s. And, finally, 76 percent of the Hispanics in the PY placebo were foreign-born, and 52 percent had arrived in the 1980s.

The large influx of new low-skill workers implies that the composition of the sample of non-Cuban high school dropouts is changing dramatically over time. Moreover, the newly arrived low-skill Hispanic immigrants had very low entry wages. A newly arrived low-skill Hispanic immigrant in 1990 earned 30 percent less than other comparably aged and educated men. The inclusion of Hispanics inevitably implies that the poor performance of the new immigrants "drags down" the average wage of the low-skill group in the affected cities, and this composition bias distorts the measured wage impact of the *Marielitos*.⁴

5. High School Students Are *Not* High School Dropouts

⁴ The new immigrants may themselves have had an additional wage impact on other workers. The size of the post-Mariel supply shock of non-Cuban, Hispanic immigration during the 1980s was 12 percent in Miami, 15 percent in the Card placebo, 13 percent in the employment placebo, and 6 percent in the PY placebo.

As the old saying goes, “old habits die hard.” The PY paper illustrates the corollary that *bad* old habits die even harder. The PY analysis represents the second time Peri has made the same error in sample construction.

Let me briefly describe the first instance. The early drafts of the Ottaviano-Peri (2006) study examined a sample of workers aged 17-65. In the U.S. context, “workers” aged 17 or 18 are almost always enrolled in high school as juniors or seniors. Because they defined a “high school dropout” as someone who lacks a high school degree, Ottaviano and Peri misclassified millions of high school students as high school dropouts.⁵

Using this problematic sample, Ottaviano and Peri (2006) then concluded that “carbon copies” of immigrants and natives (i.e., workers with the same age and education) were not perfect substitutes. The implied complementarity between carbon-copy workers, they argued, was sufficiently strong that a supply shock in any particular skill group almost always raised the wage of comparable natives. Borjas, Grogger, and Hanson (2008) showed that this conclusion depended entirely on the erroneous classification of high school students. Once these “dropouts” were excluded from the analysis, the complementarity vanished. The published version of the Ottaviano-Peri (2012) paper, which corrected for some of these problems, reports an elasticity of substitution between carbon-copy immigrants and natives that *quadruples* the elasticity reported in the original draft. Not surprisingly, Lewis’s (2013, p. 69) survey of the literature concludes that carbon copy complementarities are “very modest.”

The same error reappears in the PY analysis. In GB15, I explained the rationale for limiting the sample to persons aged 25-59 as follows:

⁵ The 1990 census indicates that 85 percent of all persons aged 16-18 are enrolled in school.

The age restriction ensures that a worker's observed earnings are not contaminated by transitory fluctuations that occur during the transitions from school to work and from work to retirement.

PY expand the sample to workers aged 16-61, but do not make *any* attempt to exclude persons enrolled in school. This age restriction means that high school sophomores, juniors, and seniors, aged 16, 17, or 18, who happen to have positive earnings are classified as dropouts. In 1990, 84 percent of men aged 16-18 with positive earnings were, in fact, enrolled in school. Obviously, the earnings of these students do not represent their true productivity, as they have nothing in common with *real* high school dropouts.

Unfortunately, the information available in the CPS on whether a person is enrolled in school is less than ideal. This is one reason why I specifically chose to avoid the problem by looking at a sample of workers aged 25-59. The CPS reports a person's school enrollment status only after 1984. The ORG reports a person's "activity last week," which could potentially be used to filter out those who spent that week in school. This filtering, however, would require that we exclude all observations collected during the summer months, cutting sample size by 25 to 30 percent, and taking away much of the rationale for using the ORG and for including younger workers as a way of increasing sample size.

It is easy to illustrate the very large impact of the misclassification of high school students as high school dropouts. To do so in the most transparent way possible, I simply calculated the average wage of all non-Hispanic men aged 16-61 in each of the relevant cities (not adjusting for age).

Figure 3 and Table 3 show what happens when we examine wage trends in the sample of men aged 16-61 and when we replicate the analysis in the subsample aged 19-61,

which gets rid of the high school students. It is evident that the trivial exclusion of “workers” aged 16-18 fundamentally changes the visual trends. Curiously, adding high school students to the sample biases the results in a way that hides the impact of the Mariel supply shock. If I use the PY placebo in the March CPS, the exclusion of the students implies an immediate wage impact of -0.265 (0.129), and this effect drops to -0.150 (0.140) when the teenagers are added to the data.

It might seem remarkable that excluding “workers” aged 16-18 would have such a dramatic effect on the wage trends. After all, there should be very few such persons in the data. This line of reasoning, however, misses the point and a little reflection shows why. There are many millions of high school students. PY wrongly classified all of these millions as high school dropouts. The large size of the high school student population implies that a remarkably large fraction of the “high school dropouts” in the PY sample are actually high school students.

In fact, the fraction of dropouts who are 18 or younger in the March CPS is 27 percent for the employment placebo and 17 percent for the PY placebo. The respective statistics for the ORG data are 24 and 18 percent. A remarkably high fraction of the sample, therefore, has remarkably low earnings. In both the March and ORG samples, the average weekly wage of the teenagers is 1.5 log points lower than the wage of workers aged 19 or more. Put bluntly, a phantom “supply shock” of *fake* high school dropouts fatally contaminates the analysis.

It is worth emphasizing that the misclassification of high school students as high school dropouts results from a deliberate change in the age restrictions used in the papers that inspired both the Ottaviano-Peri and the PY studies (Borjas, 2003; and GB15,

respectively). This carelessness in data construction always seems to lead in the same direction. In each instance, misclassifying high school students as high school dropouts generates the “finding” that labor supply shocks have little wage impact.

6. Concluding Thoughts

There are some conceptual issues that I want to address before concluding. Some readers of GB15 have argued that the timing of the wage loss suffered by low-skill men in Miami does not coincide with the timing of the Mariel supply shock. The supply shock occurred in 1980, but the largest wage loss was observed around 5 years after the shock.

As far as I know, there has been no study of the dynamics of immigration-induced supply shocks. There has, however, been much related work on the dynamics of *demand shocks* in macroeconomics. That literature recognizes and documents that the largest effects of demand shocks do not happen immediately. We only need to look back a few years and note that it took a while for the repercussions of the 2008 fiscal crisis to reverberate through the labor market. It took 2 or 3 years for the unemployment rate to peak, and another 3 or 4 years for the labor market to recover. Many studies of oil shocks, including the OPEC shock of the early 1970s, tell a similar story (Hamilton, 1983). Given the well-documented lags between demand shocks and their consequences, it is premature to claim that the dynamics of the wage data in post-Mariel Miami are not consistent with the presumed effects of a supply shock.

There is also a concern over which CPS file should be used to study Mariel, the March CPS or the ORG data. There is obviously an important difference in sample size between the two data sets:

<u>Description of sample</u>	Average annual number of observations for Miami before 1990	
	March CPS	ORG
Men, 25-59, Non-Hispanic	17.3	36.5
Men and women, 25-59, Non-Hispanic	31.2	63.9
Men and women, 25-59, Non-Cuban	50.9	92.1
Men and women, 16-61, Non-Cuban	63.9	122.9

In GB15, I tried to minimize the imprecision introduced by the small number of non-Hispanic men in Miami by effectively pooling three years of data throughout the analysis. As I have repeatedly pointed out, the search for a larger sample size—by adding women, Hispanics, or teenagers—does not excuse the fact that PY's changes in sample definition introduce various types of selection bias and composition bias. There is a very good argument for preferring a larger standard error and examining wage data that provides valid information than for having more precise estimates and examining wage trends that are confounded by many contaminating factors.

It is also worth emphasizing that the CPS and ORG files measure different concepts of income. Despite the smaller sample size in the March CPS, there are reasons to believe that the March data may be more suitable in the Mariel context. As I noted in GB15:

The March CPS reports total earnings from all jobs held in the previous calendar year. The ORG measures the wage in the main job held by a person in the week prior to the survey...The ORG does not provide any earnings information for persons who happen not to be working on that particular week, whereas the March CPS would capture the earnings losses associated with jobless periods. From the perspective of determining the labor market impact of the Mariel supply shock, it would seem that the more encompassing measure of labor market outcomes in the March CPS is far preferable.

Finally, I want to highlight a particular point made in the concluding section of PY's study that is somewhat revealing. They write:

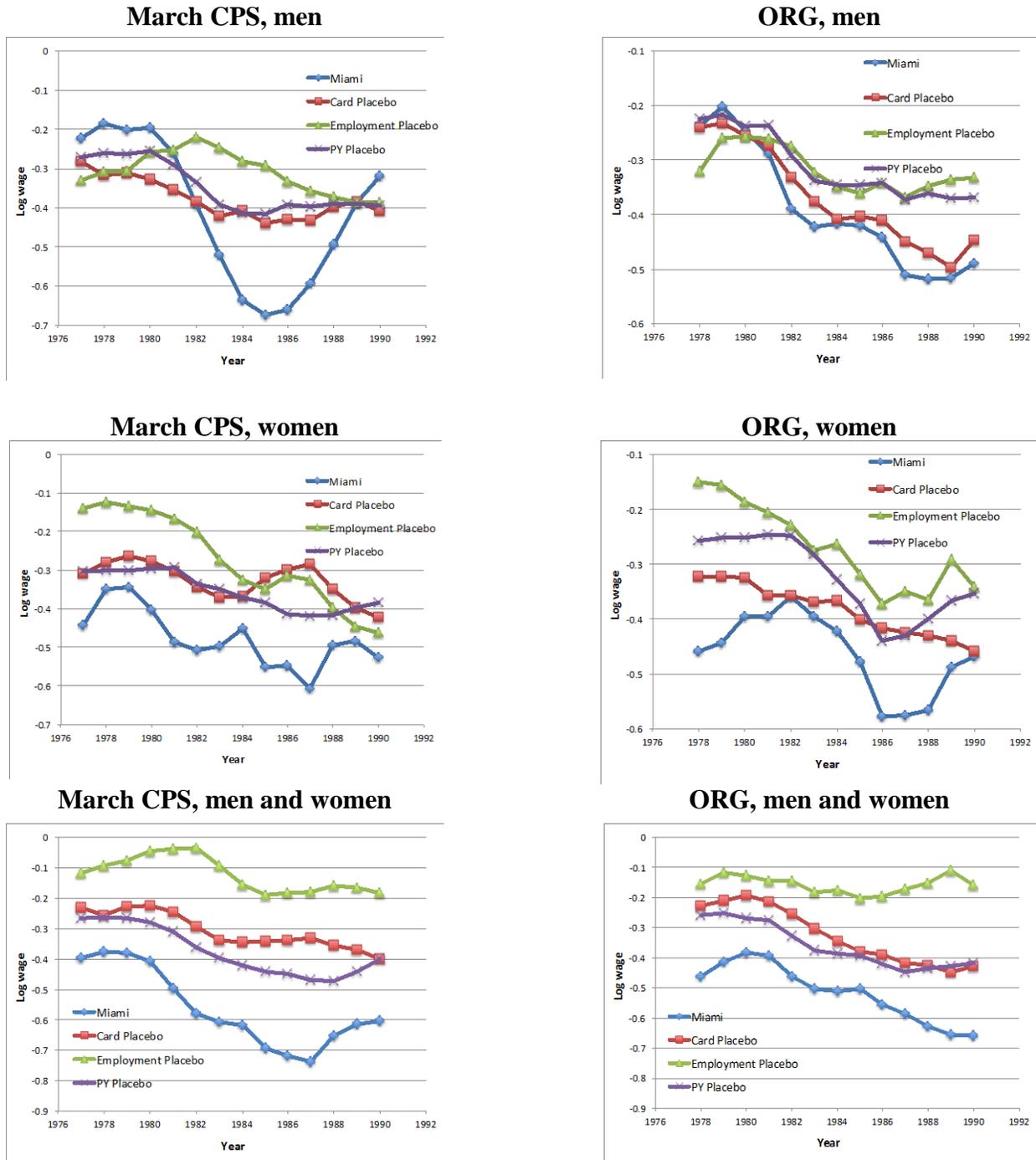
We think the final goal of the economic profession should be to agree that, even using the more current econometric methods, we do not find any significant evidence of a negative wage and employment effect of the Miami boatlift and move to analyze other cases...

I do not usually think of economists as having a “final goal” that is anything other than a careful and systematic evaluation of the evidence. The declaration that the “final goal of the economic profession should be to agree that...[PY] do not find any significant evidence of a negative wage and employment effect” is, at best, peculiar. To the contrary, I propose that PY are simply wrong; their analysis is plagued by questionable assumptions, dubious data manipulations, and one fundamental error in data construction.

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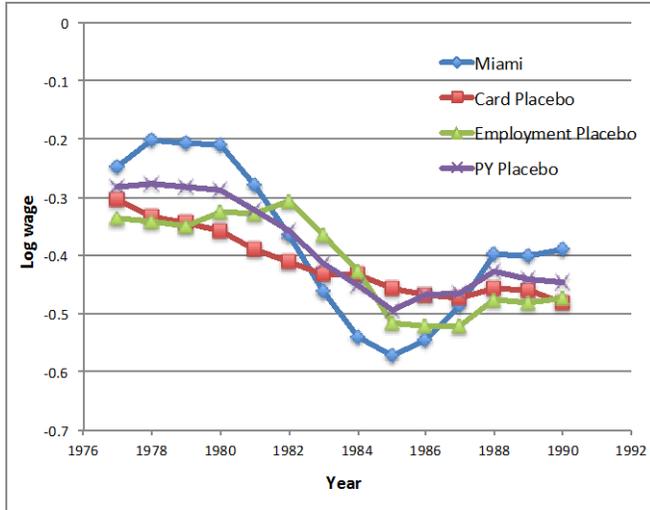
**Figure 1. The role of gender in determining the wage impact
(Sample: Prime-age workers, 25-59, non-Hispanic)**



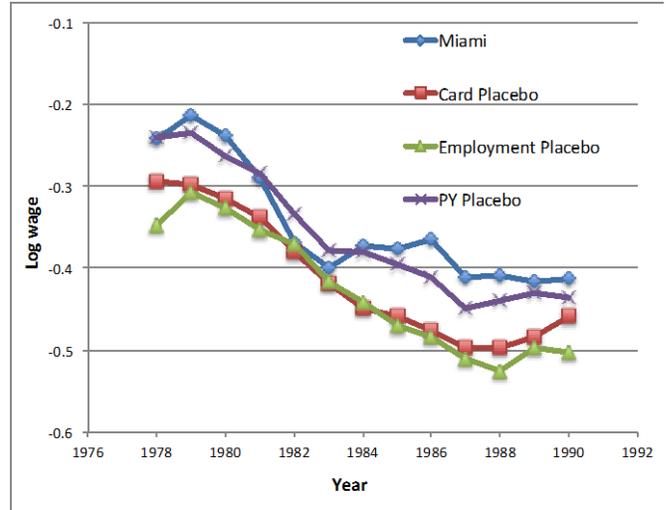
Notes: The figures use a 3-year moving average of the age-adjusted log weekly wage in each specific geographic area.

**Figure 2. The role of including Hispanics in determining the wage impact
(Sample: Prime-age workers, 25-59, non-Cuban)**

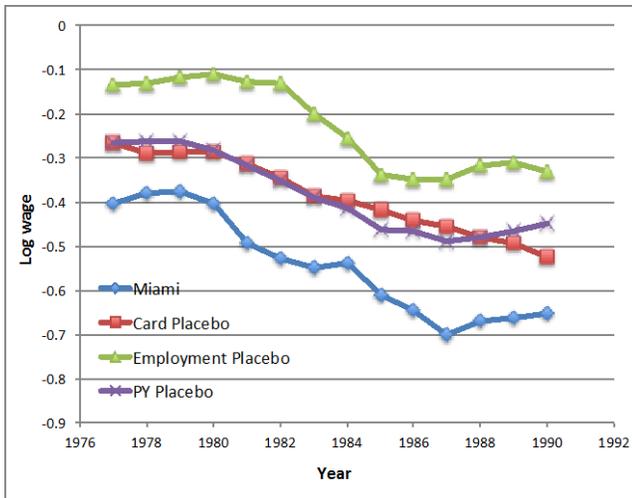
March CPS, men



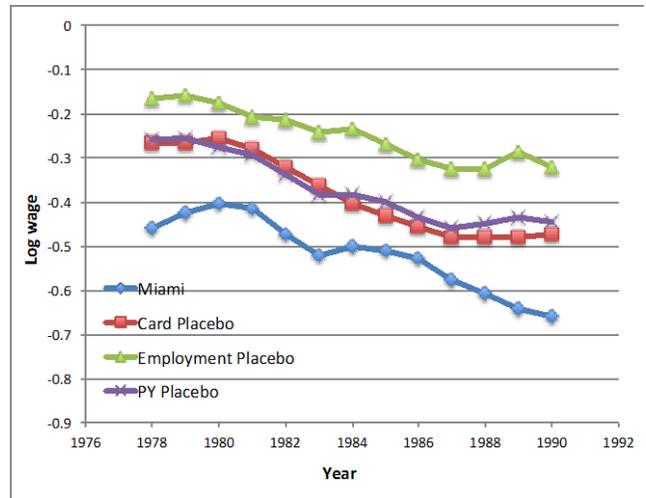
ORG, men



March CPS, men and women



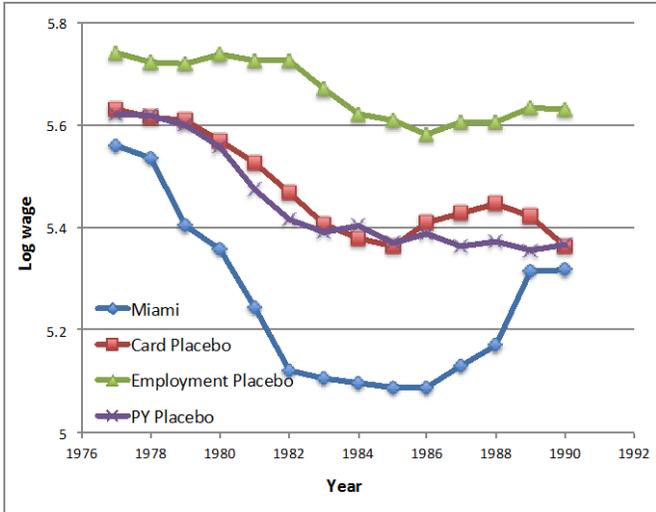
ORG, men and women



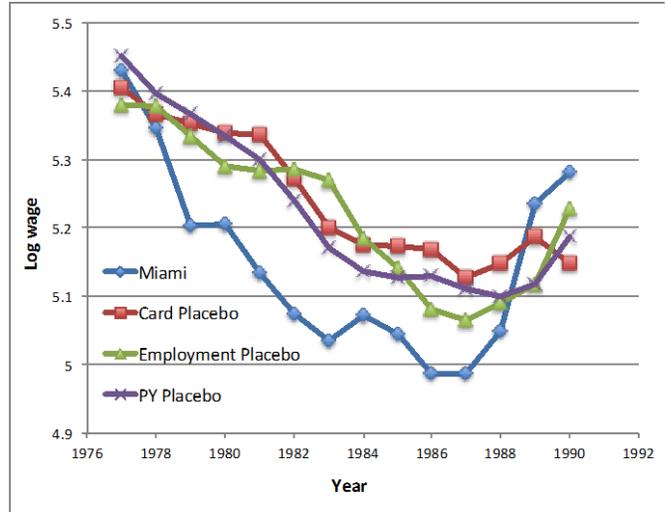
Notes: The figures use a 3-year moving average of the age-adjusted log weekly wage in each specific geographic area.

**Figure 3. The role of including high school students in determining the wage impact
(Sample: Working men, 16-61, non-Hispanic)**

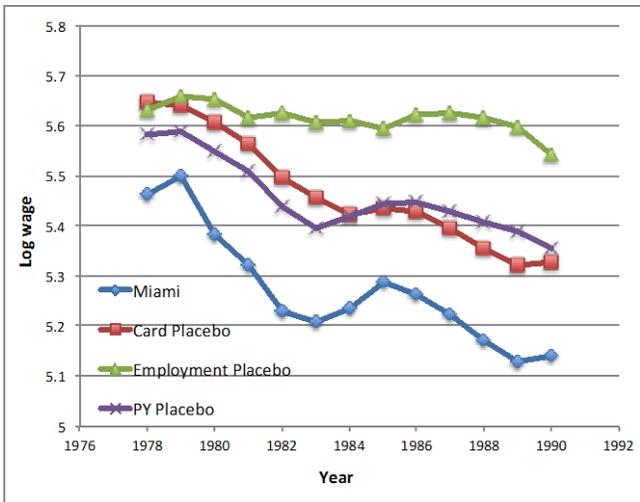
March CPS, 19-61



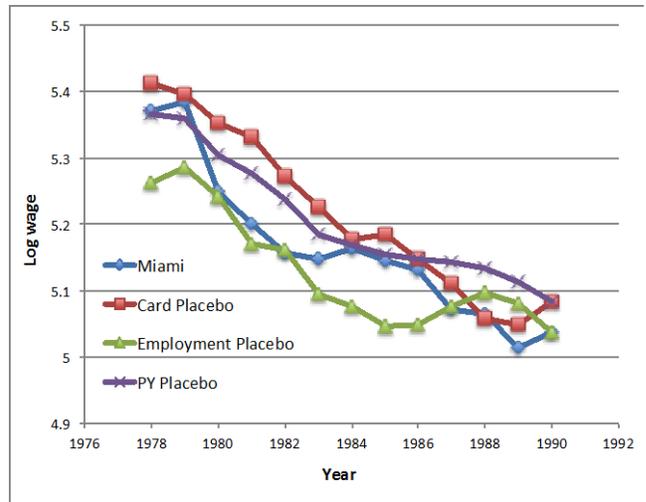
March CPS, 16-61



ORG, 19-61



ORG, 16-61



Notes: The figures use a 3-year moving average of the age-adjusted log weekly wage in each specific geographic area.

**Table 1. The sensitivity of the impact to the inclusion of women
(Sample: Prime-age workers, 25-59, non-Hispanic)**

<u>Dependent variable and treatment period</u>	<u>Card placebo</u>	<u>Employment placebo</u>	<u>PY placebo</u>
A. March CPS, men			
1981-1983	-0.137 (0.093)	-0.289 (0.090)	-0.150 (0.074)
1984-1986	-0.364 (0.080)	-0.495 (0.071)	-0.364 (0.066)
1987-1989	-0.216 (0.085)	-0.251 (0.071)	-0.207 (0.081)
1990-1992	0.188 (0.158)	0.096 (0.136)	0.134 (0.114)
B. ORG, men			
1981-1983	-0.068 (0.027)	-0.153 (0.060)	-0.131 (0.034)
1984-1986	-0.032 (0.039)	-0.097 (0.066)	-0.099 (0.043)
1987-1989	-0.061 (0.031)	-0.206 (0.055)	-0.197 (0.040)
1990-1992	0.005 (0.058)	-0.105 (0.078)	-0.084 (0.048)
C. March CPS, men and women			
1981-1983	-0.165 (0.045)	-0.253 (0.053)	-0.151 (0.046)
1984-1986	-0.272 (0.095)	-0.215 (0.083)	-0.191 (0.064)
1987-1989	-0.175 (0.092)	-0.215 (0.053)	-0.130 (0.065)
1990-1992	-0.035 (0.092)	-0.054 (0.095)	-0.114 (0.066)
D. ORG, men and women			
1981-1983	0.004 (0.033)	-0.029 (0.067)	0.006 (0.054)
1984-1986	0.065 (0.032)	-0.009 (0.056)	0.031 (0.056)
1987-1989	-0.021 (0.043)	-0.188 (0.053)	-0.087 (0.066)
1990-1992	-0.025 (0.051)	-0.148 (0.092)	-0.106 (0.066)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. The regressions are weighted by the number of observations used to calculate the dependent variable.

**Table 2. The sensitivity of the impact to the inclusion of non-Cuban Hispanics
(Sample: Prime-age workers, 25-59, non-Cuban)**

<u>Dependent variable and treatment period</u>	<u>Card placebo</u>	<u>Employment placebo</u>	<u>PY placebo</u>
A. March CPS, men			
1981-1983	-0.085 (0.057)	-0.199 (0.062)	-0.104 (0.057)
1984-1986	-0.247 (0.045)	-0.230 (0.063)	-0.186 (0.055)
1987-1989	-0.054 (0.059)	-0.101 (0.054)	-0.072 (0.066)
1990-1992	0.010 (0.062)	-0.120 (0.061)	-0.063 (0.059)
B. ORG, men			
1981-1983	-0.049 (0.025)	-0.088 (0.045)	-0.077 (0.038)
1984-1986	0.006 (0.032)	-0.025 (0.050)	-0.032 (0.042)
1987-1989	0.028 (0.034)	-0.011 (0.043)	-0.047 (0.057)
1990-1992	0.027 (0.037)	0.000 (0.037)	0.007 (0.044)
C. March CPS, men and women			
1981-1983	-0.084 (0.040)	-0.144 (0.049)	-0.089 (0.055)
1984-1986	-0.110 (0.056)	-0.048 (0.057)	-0.063 (0.043)
1987-1989	-0.083 (0.050)	-0.149 (0.045)	-0.118 (0.049)
1990-1992	0.023 (0.048)	-0.070 (0.053)	-0.114 (0.049)
D. ORG, men and women			
1981-1983	0.029 (0.028)	0.016 (0.052)	0.030 (0.046)
1984-1986	0.082 (0.035)	0.019 (0.045)	0.045 (0.048)
1987-1989	0.045 (0.033)	-0.041 (0.050)	-0.031 (0.070)
1990-1992	0.028 (0.054)	-0.040 (0.060)	-0.038 (0.055)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. The regressions are weighted by the number of observations used to calculate the dependent variable.

**Table 3. The sensitivity of the impact to the inclusion of high school students
(Sample: Working men, 16-61, non-Hispanic)**

<u>Dependent variable and treatment period</u>	<u>Card placebo</u>	<u>Employment placebo</u>	<u>PY placebo</u>
A. March CPS, aged 16-61			
1981-1983	-0.174 (0.130)	-0.181 (0.162)	-0.150 (0.140)
1984-1986	-0.122 (0.146)	-0.070 (0.147)	-0.036 (0.148)
1987-1989	-0.091 (0.133)	0.007 (0.148)	-0.011 (0.130)
1990-1992	0.319 (0.140)	0.210 (0.225)	0.219 (0.178)
B. March CPS, aged 19-61			
1981-1983	-0.261 (0.137)	-0.408 (0.111)	-0.265 (0.129)
1984-1986	-0.211 (0.155)	-0.323 (0.132)	-0.229 (0.129)
1987-1989	-0.203 (0.137)	-0.278 (0.145)	-0.171 (0.124)
1990-1992	0.213 (0.176)	0.014 (0.183)	0.164 (0.180)
C. ORG, aged 16-61			
1981-1983	-0.050 (0.062)	-0.032 (0.079)	-0.074 (0.048)
1984-1986	-0.002 (0.080)	0.061 (0.012)	0.006 (0.079)
1987-1989	0.049 (0.069)	-0.043 (0.104)	-0.069 (0.069)
1990-1992	-0.010 (0.073)	-0.010 (0.103)	-0.052 (0.062)
D. ORG, aged 19-61			
1981-1983	-0.038 (0.060)	-0.131 (0.099)	-0.084 (0.050)
1984-1986	0.058 (0.053)	-0.046 (0.078)	-0.015 (0.051)
1987-1989	0.007 (0.061)	-0.185 (0.076)	-0.117 (0.049)
1990-1992	0.003 (0.076)	-0.160 (0.109)	-0.100 (0.071)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. The regressions are weighted by the number of observations size to calculate the dependent variable.